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III.

ON A THERMO-ELECTRIC METHOD OF STUDYING CYLINDER CONDENSATION IN STEAM- ENGINES.*

BY EDWIN H. HALL.

Presented January 11, 1893.

BEFORE the time of James Watt the steam in an engine cylinder was condensed, and so gotten rid of, after it had done its work, by pouring cold water directly upon or into the cylinder. So, whenever steam was admitted for a new stroke, a great part of it was condensed in the act of reheating the cylinder wall; whence the term *cylinder condensation*. Watt invented and brought into use the independent condenser, by means of which the steam is removed from the cylinder with comparatively little cooling of the latter. It is believed, however, that even now cylinder condensation wastes a good deal of steam in common engines, whether condensing or non-condensing. The commonly accepted theory is, that during the exhaust the wall of the cylinder, to a certain depth from the steam space, or a layer of water upon the cylinder wall, becomes cooled by the evaporation which accompanies exhaustion, and that the incoming steam finds this wall, or the layer of water upon it, considerably cooler than itself. It is believed that some part of the steam which is thus condensed upon the cylinder wall during admission is recovered during the later part of the *forward* stroke by re-evaporation, but that a considerable amount, sometimes

* An informal account of much that is contained in this article was given before the Academy in May, 1891. A similar account was given before the American Institute of Electrical Engineers in New York, May 20, 1891, and was printed in the Transactions of that Society.

25% of all the steam used by the engine, is not recovered in this way, and only reappears during the *back* stroke, when it is worse than useless, as it increases the back pressure which the returning piston must overcome. Engineers have been led to this theory by a study of indicator diagrams, that is, diagrams made automatically while the engine is in operation, and recording the steam pressure within the cylinder at every point of the forward and backward stroke (see Figure 5). Such diagrams commonly show the weight of *steam* in either end of a cylinder to increase in the latter part of each stroke, as the piston moves forward after the admission valve is closed, but the greatest weight of steam shown by the indicator diagram at any part of the stroke is less than the weight of steam *and water* passing through the cylinder at each stroke. Accordingly, it is argued that a considerable part of that which comes from the boiler as steam goes through the cylinder as water, at least during that part of the stroke when it would, as steam, be of service.

But there have not been wanting some to maintain that the observed, or apparent, effect, as shown by the indicator diagram, is too large for the alleged cause. In January, 1889, Mr. Dickerson of New York, a distinguished patent lawyer, now dead, speaking before the Electric Club of New York, advanced the proposition that the peculiar character of the indicator diagram, which is supposed to show cylinder condensation and subsequent re-evaporation, is really due to leakage of steam past the engine valves, past the exhaust valve in the early part of the stroke, past the admission valve during the latter part of the stroke, the whole effect being to make the *expansion curve*, so called, of the diagram too steep at the beginning, and too nearly horizontal at the end. He made the statement that the steamers in the waters about New York City will make four or five miles an hour with the valve between the boiler and the cylinder closed. I do not know how accurate that statement was, but I have never seen it contradicted. Mr. Dickerson's argument excited my interest, and I cast about for some method of studying the problem of so called cylinder condensation, not, as engineers had done and are still doing, by means of the indicator diagram, but through the cylinder wall by means more familiar to the physicist than to the mechanical engineer. I wished to determine by actual trial how great are the fluctuations of temperature occurring during a complete forward and back stroke of an engine at a given depth of metal from the surface touched by the steam.

For this purpose I naturally had recourse to a thermo-electric device.*

The engine belonging to the Harvard Physical Laboratory, with which my experiments were made, is a Kendall and Roberts of 10-inch cylinder and 15-inch stroke, provided with an ordinary Myer's valve and cut-off. It is jacketed by an air space about 2 cm. wide.

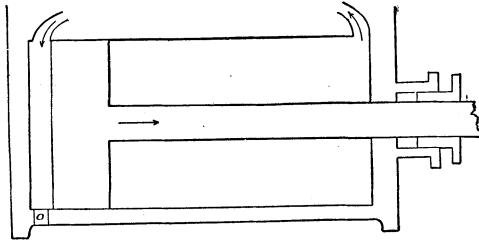


Fig. 1.

Figure 1 shows a horizontal longitudinal section of the cylinder, with the piston at the beginning of its forward stroke. The cylinder wall, about 2 cm. in thickness, is pierced at *o* for making connection with an indicator. Into this hole, which is about 2 cm. wide, I screwed a plug represented accurately enough by Figure 2. On the inner end of this plug was soldered a cover of

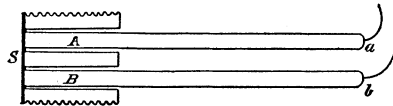


Fig. 2.

thin steel, *S*. Through two holes in the plug, and insulated from it by glass tubes, two rods extended to touch the steel cover, one, *A*, of antimony, the other, *B*, of bismuth. To the outer end of *A*, at *a*, was soldered a copper wire; to the outer end of *B*, at *b*, was soldered a similar wire. These two wires lead to a ballistic galvanometer, but at one point the electric circuit was broken, except

* A letter received from Mr. Bryan Donkin, Jr., of London, the well known engine builder, after the first account of my experiments was published, showed me that he had been before me in making experiments in the same general manner, but with small bulb thermometers in mercury cisterns instead of thermopiles. The *Bulletin de Société de Mulhouse* for 1890 contains some account of his work. My results appear to be not discordant with those which he obtained.

when, at any desired part of the stroke, it was closed for a short time by the revolving crank of the engine. With this apparatus there should be a current, and therefore an effect upon the galvanometer, at closure of the circuit, so long as the two inner ends of the antimony and bismuth bars, which press against the steel, are at a different temperature from the outer ends, which are placed side by side in a pot of melted paraffin or hot oil. But when, on the other hand, there is no effect on the galvanometer at closure, one may, if proper precautions have been taken, conclude that the temperature at that surface of the steel which is touched by the antimony and bismuth, to determine which is the object of the experiment, is the same as that of the heated liquid surrounding the ends *a* and *b*, which a thermometer at once makes known.

This represents one stage of the investigation, but as I gradually became convinced that considerable fluctuations of temperature really occurred at measurable depths of the cylinder wall, I saw that a better considered thermopile than the one at first used was needed. I must discard the steel, the thermal conductivity of which might differ considerably from that of the cast-iron composing the cylinder wall, and must replace the antimony and bismuth by something not very different from cast-iron in thermal conductivity, for it is evident that the temperature existing at any moment at the surface of contact of the iron and the metal outside it may depend greatly upon the conductivity of this second metal. Moreover, this surface of contact should be of considerable breadth, and the contact very perfect; in short, everything should be done to make the surface of contact resemble in all its conditions, as nearly as might be consistently with the object of the experiment, the surface of contact of two strata of iron within the actual cylinder wall. Accordingly, I made the plug and the cover, which now was fastened by four small steel screws to the inner end of the plug (see Figure 3), from the same bar of cast-iron, — not ordinary cast-iron, which would be too brittle for the thin cover, but cast gun-iron of density 7.18, which I found by experiment to have a thermal conductivity not very different from that of ordinary Southern cast-iron of density 7.06. In place of the antimony and bismuth bars I now used a single cylinder of cast nickel of density 8.08, the thermal conductivity of which I have found by experiment to be very nearly the same as that of the cast gun-iron at 115° or 120° Centigrade. The specific heat of nickel is about the same as that of iron.

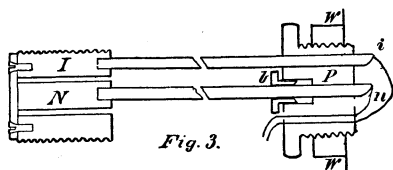
Notwithstanding the great similarity of cast-iron and cast nickel in many respects, they make a powerful thermo-electric couple, so that nickel appeared to be of all substances the one best fitted for my purpose. The hole bored in the iron to receive the nickel cylinder was about 0.65 cm. in diameter. The cylinder itself was about 0.025 cm. less in diameter, so that when wrapped with a single layer of ordinary printing paper it would fit rather snugly in the hole. The cast-iron plug with its cap in place was about 2.2 cm. long, and the nickel cylinder projected very slightly beyond the outer end of the plug.

The inner end of the iron plug, the inner end of the nickel core, and that surface of the iron cap which was to press against them, were all worked with great care to a plane polished surface. The cap and the nickel core were then soldered together in the following manner. That part of the cap surface which was to meet the nickel was thinly tinned with ordinary solder and then the cap was screwed firmly into place upon the plug. That end of the nickel which was to meet the iron was also tinned, and then this core, wrapped, save at the ends, by a single layer of paper, was thrust into the hole in the plug. The plug with its contents was then heated very hot, and while hot was placed in a vise between blocks of wood, which by the action of the vise pressed the nickel core and the iron cap so close together that the layer of solder left between them was, according to my measurements, less than 0.002 cm. thick. Then, after cooling, the screws holding the cap in place were drawn out, and cap and core were carefully removed from the plug. The excess of solder which had been pressed from between them, and which had gathered about the base of the core, was then carefully turned off in a lathe with a brass tool. The core was then wrapped with fresh paper and put back in place, the cap being carefully screwed on. Red lead was used around the screws holding the cap, but I depended mainly upon the perfection of the contact between cap and plug to prevent leakage at this joint.*

* In some of the experiments this joint was not perfectly tight, for water was sometimes seen to come out past the core, *N*, before the plug became hot. Generally upon such occasions the leaking appeared to cease when the plug became hot. The fact probably is that the joint continued to leak slightly, but leaked dry steam, the temperature at the outer end of the plug being about 110° C., and at the inner end considerably higher. Both experiment and reason indicate that the effect of such leakage upon the temperatures observed was slight.

Three plugs were thus completed, the caps being respectively 0.051 cm., 0.101 cm., and 0.203 cm. thick. All the iron parts were from one bar, and all the nickel parts were from one bar.

From the nickel core of each plug when in use extended a slender bar of nickel about 15 cm. long, made from the same piece as the core, and from the outer end of the iron plug extended a similar bar of iron made from the same piece as the plug. The bars were not rigidly fastened to the nickel core and the iron plug, but were held by friction in holes bored to receive their ends. The outer ends, *i* and *n*, Figure 3, extended through a plug of hard rubber, *P*, screwed into the thickened wall, *WW*, of a vessel containing melted paraffin, and copper wires soldered to these ends led back through the hard-rubber plug toward the galvanometer, which was about 60 m. distant. To facilitate adjustments the central hole of the

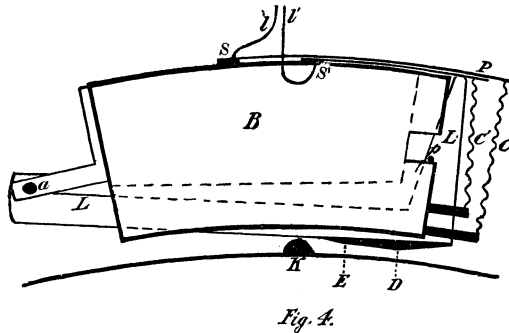


hard-rubber plug was made somewhat too large for the nickel connecting bar reaching through it, and a stuffing-box, *b*, was depended on to prevent leakage past this bar. The bulb of a thermometer hung near the junctions *i* and *n* in the melted paraffin, which was heated by a lamp placed below and was stirred by a mechanism driven by the engine.

The key by means of which the electric circuit could be closed for a short part of each stroke required some thought in its evolution, and will be described with detail, although no doubt it might be improved.

At one stage in my experiments I tried making contact by means of a fork-shaped spring of brass, which revolved with the engine crank, and at one part of the stroke bestrode a gap in the circuit, rubbing with each prong upon brass. A certain amount of thermoelectric force was to be expected at each of the two rubbing points of contact, but I had hoped that the two forces would counteract each other sufficiently for my purpose. In this I was disappointed, and I afterwards endeavored to avoid rubbing at the point of closure of the circuit. The key finally used was essentially like

that shown in Figure 4, in which B is a block of hard wood about 14 cm. long on the upper curved edge, 6 cm. thick, and 5 cm. wide. The brass spring S fastened to the top of the block is connected by means of the wire l with the galvanometer. The brass spring S' also fastened to the top of the block, is connected by means of the wire l' with one of the junctions i or n in the pot of melted paraffin. The two springs do not at present touch each other, but will do so at P , as soon as S' is lifted by the lever LL , which is pivoted at a , and will be struck at E by the cam K on the engine crank. The lever LL works in a narrow slot sawed in the block B . To prevent short circuiting through the body of the engine this lever



is tipped with hard rubber where it touches the spring S' . The electromotive force which one has to deal with in this experiment is so small that a mere touch of S' against S is not sufficient. Contact must be maintained for a short time, and for this purpose the lever LL is so shaped that the cam K rubs against it all the way from E to D , about one sixtieth of a revolution, one sixtieth of a second in my experiments. To prevent illegitimate contacts or illegitimate breaks between S and S' , each of these strips is controlled by two stiff spiral springs, which are indicated by C for S , and by C' for S' . The motion of S is, moreover, limited by stops, not shown in the figure, placed above and below at the end. The lever LL is made of thin sheet brass in order that its momentum may not be troublesome, but the part ED , along which contact with K endures, is reinforced by a narrow strip of iron soldered on. A pin, p , keeps LL in contact with S' when both are at rest, and prevents the lever from dropping so far as to be struck too hard by K .

To prevent thermo-electric troubles I used continuous wires from this key to the distant room where the astatic galvanometer was placed. This was an instrument of tolerable, but not extraordinary, sensitiveness. Its resistance was probably about five ohms, and the time of a single vibration about six seconds. I have spoken as if the electric circuit were closed at every stroke of the engine by the key just described. In fact, the circuit was closed only when this key and another under the hand of the observer at the galvanometer were in operation simultaneously. The method of procedure did not require measurement of the galvanometer deflections. It merely required the observation of that temperature in the paraffin pot which should make the galvanometer deflections zero. The period of vibration of the needle was so much greater

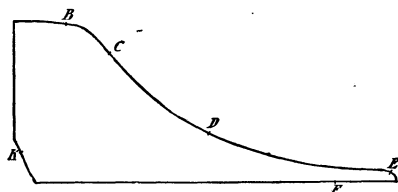


Fig. 5.

than the time of a stroke of the engine, that, by holding his key down for several strokes in succession, the observer at the galvanometer could magnify any effect produced upon the instrument, and therefore determine more sharply when the desired condition of no effect was reached.

The method of work was somewhat as follows. Everything being in readiness, the observer at the galvanometer would satisfy himself by trial that the closed circuit, *with the iron-nickel thermopile omitted*, had no perceptible effect upon the galvanometer. He would then signal for the thermopile to be brought into action, and, watching the galvanometer, would determine whether the paraffin was too hot or too cold for equilibrium, and signal the other experimenters accordingly. If it was too cold, finely divided paraffin was thrown into the pot, while some of the overheated liquid was drawn off through a cock at the bottom. The following set of observations was made on April 17, 1891, with the 2 mm. thermopile, contact occurring near the point of cut-off (Figure 5, C).

Time.		Temp. of Paraffin.
3:16	Thermopile out. No certain effect.	
	Thermopile in.	o
3:21 $\frac{1}{4}$	Effect uncertain.	130.0
3:22 $\frac{1}{2}$	Scale to right.	130.4?
3:23 $\frac{1}{4}$	" "	133.6
3:24 $\frac{1}{2}$	" "	137.7
3:26 $\frac{1}{4}$	" "	133.2
3:27	" "	131.4
3:27 $\frac{1}{2}$	Effect uncertain.	129.5
3:28 $\frac{1}{4}$	" "	126.5
3:28 $\frac{3}{4}$	Scale to left.	125.0
3:29 $\frac{1}{4}$	" "	123.6
3:30 $\frac{1}{2}$	" "	124.4
3:31 $\frac{3}{4}$	" "	126.7
3:33 $\frac{1}{4}$	Scale to right.	130.6
3:34 $\frac{3}{4}$	" "	131.0
3:35 $\frac{1}{4}$	" "	129.5
3:36	Effect uncertain.	127.5
3:36 $\frac{3}{4}$	Scale to left.	125.5

This was the first set of observations made after a rather important change in the apparatus, and shows lack of skill. The next series, made the same day, is somewhat better. The same thermopile was used, but contact was during the latter part of compression, just before admission of new steam, near the point *K* in Figure 5.

Time.		Temp. of Paraffin.
3:50	Thermopile out. No certain effect.	o
3:52	Effect uncertain.	124.1
3:53 $\frac{1}{2}$	" "	125.0
3:54	Scale to right.	124.9
3:54 $\frac{1}{2}$	" "	125.0
3:55 $\frac{1}{2}$	" "	127.0
3:56 $\frac{1}{4}$	" "	126.2
3:57 $\frac{1}{4}$	" "	125.2
3:57 $\frac{3}{4}$	" "	124.6
3:58 $\frac{1}{2}$	" "	124.0
3:59 $\frac{1}{2}$	Scale to right, slightly.	123.5
4:0 $\frac{1}{8}$	" ?	?
4:1	Scale to left.	122.8
4:1 $\frac{3}{4}$	" "	122.3
4:2 $\frac{1}{2}$	" "	122.0
4:3 $\frac{1}{4}$	" "	121.8
4:4 $\frac{1}{2}$	Effect uncertain.	122.5
4:6 $\frac{1}{4}$	Scale to right.	124.3

While such observations as those just recorded were being made, one observer was usually engaged in taking indicator diagrams from

Date.	Part of Stroke.	Point in Fig. 5.	Temperature at Depth of		
			0.051 cm.	0.101 cm.	0.203 cm.
	Inches.				°C
April 17	3.31- 3.94	C	129.5
" 17	14.84-14.97	E	129.
" 17	0.22- 0.06	K	124.5
" 24	14.84-14.97	E	°C. 123.0		
" 24*	0.22- 0.06	K	122?		
" 27	3.31- 3.94	C	133.0		
" 27	12.53-12.00	F	119.0		
" 27	0.22- 0.06	K	119.5		
May 6†	12.53-12.00	F	124.0		
" 8	12.53-12.00	F	124.0		
" 8	1.78- 2.38	B	136.0		
" 8‡	7.25- 7.94	D	122.5		
" 11	3.31- 3.94	C	. . .	°C. 135.5	
" 11	7.25- 7.94	D	. . .	134.0	
" 11§	14.81-14.94	E	. . .	129?	
" 15	14.31-14.94	E	. . .	128.5	
" 18	3.81- 3.94	C	°C 128.0
" 18	14.81-14.94	E	126.5?
" 18	0.22- 0.06	K	123.5?
" 18	0.22- 0.06	K	123.0

* In this set of observations a substitute took the place of the usual observer at the galvanometer.

† The thermopile leaked badly past the nickel core during this set of observations. It was put in order before the observations of May 8, when it appeared to be perfectly tight.

‡ The temperature of equilibrium appeared to fall in this set of observations from near 123° to near 121°.

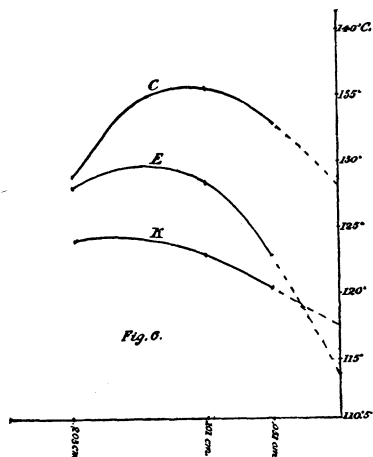
§ From a short set of observations in which the temperature was not well controlled.

|| Nickel connecting bar found apparently loose at connection with core at end of series of observations.

the engine, and noting at frequent intervals the number of strokes per minute. The general type of the diagrams is shown in Figure 5. The maximum pressure during admission was generally near 34 lb. above atmospheric pressure, and cut-off occurred near one quarter stroke. Wishing to have the diagrams affected as little as possible by the action of the governor, I depended largely upon the load, which was a dynamo with closed circuit, to regulate the speed of the engine to sixty strokes per minute. The steam pressure, however, was by no means constant, and the speed sometimes rose or fell two or three strokes from the desired figure. This inconstancy, and the discrepancy of thermometers, one or two of which were broken during the experiments, may help to account for some lack of consistency in the results obtained, which are presented in tabular form on page 46, estimated stem corrections having been applied to the thermometer readings.

I have omitted from this table all results of observations made previous to April 17, 1891, for the reason that in these earlier observations the time during which contact lasted was about three times as long as in later ones. Nevertheless, the results obtained with the long time of contact are in general agreement with those obtained later, and as in the table there is no record for the depth 0.101 cm. at that part of the stroke marked by *K* on the indicator diagram, I shall fill this gap, as well as I can, from a comparison of the earlier observations with the later, which comparison indicates the temperature 123°. I shall undertake to show by means of a diagram, Figure 6, the temperature condition of the inner part of the cylinder wall as indicated by my observations at three points of the stroke, *C*, *E*, and *K*. Distances into the cylinder wall from its inner surface are laid off along the base line of the diagram from right to left. Temperatures in excess of 110°.5, the temperature of the outer surface of the cylinder wall, are measured upward from this base line. Line *K* indicates the condition of things just before admission of new steam; line *C*, the condition at or near the time of cut-off; line *E*, the condition near the end of the forward stroke. The broken parts of curves are extensions beyond the region of actual observation. It is easy to see that an error of one or two degrees in the observation of temperature at the depth 0.051 cm. would make a great difference in the position of the broken part of the curve to which this observation belongs, and it is evident that the broken part of curve *C* cannot be correct, for it indicates at the inner surface of the cylinder wall a temperature some degrees

lower than that of the steam in the cylinder at this instant, as calculated from the indicator diagram. We cannot suppose this possible, for during the admission of steam this inner surface must have become very nearly as hot as the steam itself, else the interior strata of the iron could not have become heated to the extent observed, and the inner surface could not, after being thus heated, cool faster than the steam in front of it and the iron behind it. The same objection cannot be made to curves *E* and *K*, yet the broken parts of these curves are open to much doubt, and it is not certain that they should cross each other. A much more accurate determination of temperatures than I have made is needed to draw



any one of the three curves with confidence. Moreover, observations at a depth of 3 mm. are desirable, though under the conditions of my experiments the fluctuations of temperature at that depth are probably not more than one or two degrees.

Disregarding inaccuracies of the diagram, I shall now undertake a rough estimate of the amount of heat contained by the cylinder wall at the instant of complete cut-off, as indicated by the curve *C*, in excess of the amount which it contained just before admission, as indicated by curve *K*. The planimeter will show that the average temperature along that part of curve *C* shown in the diagram exceeds the average temperature along that part of *K* there shown by nearly 12° Centigrade. This is equivalent to a uniform elevation of about $2^{\circ}.4$ extended to a depth of 1 cm. Taking the

specific gravity of iron to be 7.2 and the specific heat to be 0.113, we find that through each square centimeter of the inner surface of the cylinder wall exposed to the steam during the *whole* time of admission, there have entered $2.4 \times 7.2 \times 0.113 (= 1.95)$ c. g. s. units of heat. I estimate the surface exposed to steam in cylinder and port when admission *begins*, to be 1700 sq. cm. If we estimate the piston face and port surfaces to be only two thirds as effective for condensation as an equal surface of cylinder head, we may reduce this area to about 1400 sq. cm. The movement of the piston *during* admission exposes about 700 sq. cm. more of the curved cylinder surface. If we count this as being equivalent to 350 sq. cm. exposed during the whole of admission, we have $1400 + 350 (= 1750)$ sq. cm. as the *effective* area of the surface upon which condensation occurs during admission. The whole amount of heat absorbed through this surface in this time is, then, $1750 \times 1.94 (= \text{approximately } 3400)$ c. g. s. units. This would correspond to the condensation, at 49 lbs. absolute pressure, of $3400 \div 510 (= \text{approximately } 6.7)$ grams of steam. The weight of *steam* in the cylinder at the end of the forward stroke was, according to the indicator, about 13.5 grams. The weight of steam in the cylinder at cut-off was considerably less than this, probably about 10 grams. According to this calculation, then, the amount of steam *condensed* during admission was about two thirds as much as the amount not condensed, and about one half of the condensed portion was re-evaporated during expansion. The amount re-evaporated during the exhaust was probably much less, for the curves of Figure 6 indicate that much less heat flowed back to the inner surface during exhaust than during expansion, and we know that a very considerable amount of heat passes *through* the wall.

A very rough estimate of water consumption, made May 8th, indicated that about 23.5 grams of water and steam passed through each end of the cylinder at each stroke. The calculations, or conjectures, just made account for rather less than three fourths of this, but the discussion goes to show that the ebb and flow of heat indicated by these thermo-electric experiments is of the right order of magnitude to account for a large part of the cylinder condensation, and to encourage the hope that a careful and extended set of such experiments would yield results of great certainty and value. I am not prepared to undertake such a labor at present, but the thermopiles which I have used in this preliminary investigation

have now been placed in the hands of two students of mechanical engineering in a well known Professional School, and I look for interesting results from their work.

For indispensable assistance in the work which this article describes I am indebted to Messrs. Barron, Curtis, Hale, Kendrick, and Page, members of a class at Harvard College engaged in a study of the steam-engine.